

things as orbits, but that the planets sweep on through a pathless void, in directions perpetually changed by gravitation."

It remains to remove a misapprehension of Prof. Meldola's own. He says: "It may be well to indicate that many—perhaps we may say the majority of biologists in this country—have long ago parted company from Mr. Spencer on this question of the enhanced importance of 'direct equilibration,' and the subordinate position assigned to 'indirect equilibration' in his later writings." I know of no foundation for such part of this statement as refers to my opinions. That I have not changed my view concerning the respective shares of direct and indirect equilibration there is incontestable proof. If Prof. Meldola will turn to § 170 (p. 468) of the "Principles of Biology," first edition, he will find it there contended that at first "natural selection worked almost alone in moulding and re-moulding organisms into fitness for their changing environments; and natural selection has remained almost the sole agency by which plants and inferior orders of animals have been modified and developed." He will find it further said that in proportion as organisms become complex and active, "the production of adaptations by direct equilibration takes the first place—indirect equilibration serving to facilitate it." And now, if he will turn to the revised edition of the "Principles of Biology" issued last year, he will find that the two sentences quoted stand as they did in 1864.

Prof. Meldola has been misled by a not unnatural illusion. I have of late years had occasion frequently to insist on the share taken in organic evolution by direct equilibration (or the inheritance of functionally-produced changes) because it has been continually denied; and frequent insistence on its share has been mistaken for an alleged extension of its share.

Brighton, January 8.

HERBERT SPENCER.

Carte Géologique internationale de l'Europe.

I HAVE just received, through Mr. Edward Stanford, *Livraison iii.*, containing seven sheets of this great work, in which we have the maps of the British Isles, of Germany, Italy, Austro-Hungary and Greece. These maps exhibit a marvellous amount of care on the part of the Directors, and of elaborate execution on the part of Herr Dietrich Reimer and his staff. I do not venture to eulogise, much less to criticise the maps, but only to direct attention to one special point of interest which they exhibit, namely, the representation of the great terminal moraines on both sides of the Alps. It requires a little close scrutiny to discover the course of these great banks of glacier detritus laid down at the epoch of greatest cold of the Glacial Period; but once recognised, it amply repays attention to follow their course. They are represented by lines of purple dots, about three or four deep, lessening in size inwards; and, of course, passing disconnectedly over all the geological formations. On the north base of the Alps the moraine bank starts from above Grenoble, swelling northwards near to Lyon along the Rhône valley; then, retreating southwards, it winds along the flanks of the Jura above the plain of Geneva and Lake Neuchâtel; then passing by Berne and Zürich, stretches away northwards by Schaffhausen and Ravensburg, indicating the enormous extent of the old Rhein Glacier; and then curving outwards along the valleys descending from the eastern Alps by Munich and Salzburg, it is represented as surrounding the northern shore of the Traun See; but no further eastwards.

The northern limit of erratic blocks is represented by a nearly continuous red line extending generally much further from the base of the Alps than is the case with the terminal moraines. Commencing on the west at Grenoble the line curves round by Lyon and Bourg, and then ranging along the Jura Meridional, celebrated for the huge boulders of granite which are there stranded, stretches northwards towards Besançon; thence, skirting the Rhein valley near Basel, the line stretches eastward by Schaffhausen, where it almost skirts the northern base of the moraine, and so passes onwards by Munich to the banks of the Enns by Steyr (Steyer).

The great glaciers which descended from the Alps on the Italian side have (as is well known) left behind them huge moraines, which are also represented in a manner similar to those on the north side. Thus we have the great terminal moraines of the rivers Dorea Riparia and Dorea Baltea near Turin; then, further eastward, those which border the southern shores of Lakes Maggiore, Como, D'Isco and Garda. The southern limit of erratics is only represented at a few places, and then

generally in close proximity to the margin of the moraines. The close connection of the great Italian lakes with the moraines cannot be overlooked by those who recognise the evidence adduced by the late Sir A. C. Ramsay in support of his views on the glacial origin of lakes. The moraines of the Dorea Riparia and Dorea Baltea were amongst his favourite illustrations.

Before closing I might be allowed to add that the topography of the map is admirable, while the coloration of the geological formations, except, perhaps, in the case of the British Isles, fully sustains the reputation of the Lithographic Institute of Berlin.

EDWARD HULL.

January 3.

Periodic Tides.

CAPT. A. S. THOMSON (p. 125) calls attention in your columns to the subject of short period oscillation of water-level at Malta and Sydney, and asks for further information from others. As I have given some attention to studying similar phenomena on the eastern coast of Canada, I venture to offer the following additional information and suggestion of an explanation.

(1) The phenomena are very common. At St. John, New Brunswick, on the coast of the Bay of Fundy, the oscillations have a fairly constant period of 43 minutes. At Quaco, a few miles further up the bay, the period is only 12½ minutes. At Halifax, Nova Scotia, on the Atlantic coast, the period is 23½ minutes. In the Gulf of St. Lawrence, at South-West Point (Anticosti), the oscillations are rapid but irregular; at St. Paul's Island, very rapid and irregular; at Forteau Bay, small and irregular; at Carleton (Quebec) there is some indication of a 22-minute period; at Souris (Prince Edward Island) the oscillations are rapid and irregular; at Pictou (Nova Scotia), small and irregular; at St. Peter's Island, very rapid and irregular.

(2) Any explanation must account for two distinct things: the *origin* of the fluctuations, and their *periodicity*. Let us take these in reverse order.

(3) The period of the oscillations (where they have a definite period) is, I believe, simply the period of the free natural vibrations of a semi-confined body of water "wish-washing" to and fro like water in a wash-bowl, the oscillations being sometimes *fundamental*—that is, consisting in the vibration of the body of water as a whole; and in other cases (perhaps the majority of cases) *partial*—that is, due to the body of water dividing up into two vibrating halves, or three-thirds, &c. In a very irregular basin, like the Gulf of St. Lawrence, regular vibrations are impossible. In some other cases the basin is of sufficiently regular form to admit of fairly regular oscillations, but not regular enough for the period to be deduced mathematically. In only two cases have I found a mathematical test possible. At St. John, the Bay of Fundy is bounded on one side by the slightly indented New Brunswick coast, and on the other side by the straight, abrupt Nova Scotia coast, these two shores being only slightly inclined to one another. The width may be taken as forty miles, and a study of the chart gives the mean depth at low tide as 34.4 fathoms. The period of fundamental vibrations across such a basin is given (to a sufficient approximation) by

$$t = \frac{2l}{\sqrt{gh}},$$

l being the width, and h the mean depth. With the above figures, this gives for fundamental vibrations a period of 87 minutes, and for first partial vibrations a period of 43.5 minutes. The latter is remarkably close to the observed period of 43 minutes. It should be noted that the calculation applies to low tide. Now, the tide at St. John has a range of 20 feet, while the formula shows that the period varies inversely as the square root of the mean depth. A simple calculation shows that the period at high tide should be two minutes less. From an examination of all the cases available, I found that the mean period for high tide was actually 1.6 minutes shorter than for low tide; but the fewness of the well-marked cases available to me for making this test (thirteen in all) makes me believe that this agreement is somewhat accidental.

The other case known to me to which a similar calculation can be applied, is that of a small basin in the St. John River, just before it flows through a very narrow gorge into the harbour. While using a rough form of self-recording tide gauge for finding accurately the time of high water, I discovered on the

record, not only the above-mentioned 43-minute oscillations, but also a series of much more rapid oscillations having a period of about 35 seconds (the latter figure being obtained by a stop-watch). The width of the basin is, at the point of observation, 2030 feet, and its mean depth about 100 feet (the tidal range is less than 2 feet). Calculation by the above formula gives, for first partial vibrations, a period of 35.8 seconds, which is very close to the 35 seconds observed; but it is to be noted that for a basin whose depth is *not* small compared with its width, the above formula is somewhat in error, and a more correct formula (involving a hyperbolic cotangent) gives 37.5 seconds. In any case the agreement is striking, and the two instances given are almost conclusive as to the explanation advanced. Simultaneous observations on opposite sides of such a basin would be quite conclusive, but these I have not yet had an opportunity to make.

(4) The following notes on the other cases referred to in (1) may be of interest in connection with observations referred to by Capt. Thomson. Quaco is only about twenty miles further up the Bay of Fundy than St. John, and yet, while expecting to find there a period similar to that at St. John, I found a period of 12½ minutes (from four separate records). This was at first puzzling, but, later, an examination of the chart showed the existence of a dangerous ledge (the Quaco Ledges) coming nearly to the surface, at a distance of eight or ten miles off shore, and forming, with a headland above Quaco and another below, an irregular basin, the dimensions of which no doubt determine the period of oscillation. A quite similar explanation applies to the oscillations at Halifax, for there a succession of banks (the Emerald, Sable Island, Le Have and Roseway) form, with the Nova Scotia coast, a large-sized bay of irregular shape.

(5) As to the external impulse that starts the oscillations, there is much uncertainty. Marked oscillations at St. John are frequently accompanied by barometric disturbances, but not always. My own observations make me believe that the oscillations and heavy ground-swell usually coexist. A notable case (for which I have to thank Mr. S. W. Kain, of St. John) occurred on September 18, 1898, when the heaviest ground-swell in several months was accompanied by marked periodic oscillations recorded on the Kelvin gauge. On the whole, I believe that the disturbance of equilibrium is due either to abrupt local variations of atmospheric pressure, or to the transmitted effect of a distant hurricane.

Those who are interested in this subject will find fuller details of the cases here discussed numerically (and also a short bibliography of the subject) in a paper by myself in the *American Journal of Science* (vol. iii., 1897). The nature of the oscillations in the Gulf of St. Lawrence are shown on curves that illustrate a tidal report, by Mr. W. Bell Dawson, in the last volume of *Transactions of the Royal Society of Canada* (see *NATURE*, vol. lviii. p. 260). A. WILMER DUFF.

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GENERAL LAW OF THE PHENOMENA OF MAGNETIC PERTURBATIONS OF SPECTRAL LINES.

IN the *Philosophical Magazine* for April 1898, I pointed out that the resolution into triplets, &c., which the spectral lines suffer in a strong magnetic field, did not appear to follow any obvious general law, but appeared to be some complex function of the wavelength. To this was added the following remark:—"It is possible, however, that the lines of any one substance may be thrown into groups for each of which $\delta\lambda$ varies as λ^2 , and each of these groups might be produced by the motion of a single ion. The number of such groups in a given spectrum would then determine the number of different kinds of ions in the atom or molecule.

"Homologous relations may also exist between the groups in different spectra, but all this still remains for complete investigation."

Although this investigation is still far from complete, yet the measurements so far made uniformly go to show that the foregoing expectation is about to prove true, and that e/m or $\delta\lambda/\lambda^2$ is the same for the corresponding

lines of the natural groups in the same spectrum, and, further, that this quantity remains the same for corresponding lines or groups in the homologous spectra of different substances.

Not only is the magnitude of the magnetic effect governed by the foregoing law, but the *character* also of the effect is the same for the corresponding lines; and this is very interesting, as it shows that the corresponding lines probably arise from the same origin. The theory is consequently verified by the facts when the spectral lines are considered in groups corresponding to the molecular events which produce them.

THOMAS PRESTON.

COAST-TELEGRAPHS AND SPACE-TELEGRAPHY.

THE year 1898 was an important period in the history of space-telegraphy, it was the period in which the possibility of being able to signal across wide stretches of open sea, with certainty in all weathers and at high speeds, became first generally recognised as practicable. Within the year the final report of the Royal Commission on the question of Coast-Telegraphs, published late in 1897, came into our hands; and the last few months of the year witnessed a truce to the war of "wireless-telegraphy." A wave of good feeling has now united the opponents into something like coherence, and the honours have been divided with universal approval. The result is that for the future Italy takes prominence, England eminence, while Russia, Germany and France share the luxury of many grievances.

The close of the year is very appropriately characterised by three papers, respectively communicated by Dr. Lodge,¹ Mr. W. H. Preece,² and Mr. S. Evershed,³ to the Institution of Electrical Engineers, all emphasising the merits of one and the same system of space-telegraphy. The authors themselves were more or less unanimous as to the course further experiment should take, but the discussion that followed the reading of these papers showed a certain lack of directness; many of the speakers were carried away by side issues, and a great deal of time was occupied with ill-considered suggestions and old matter. While fully recognising the value of open discussion, and of hints thrown out at random on subsidiary matters, the present writer thinks it may be useful here to indicate the limits to which the problem may be narrowed down, and to point out the very serious work that is now calling for the aid of space-telegraphy.

It is very generally admitted that space-telegraphy will replace metallic-circuit systems only under conditions where metallic circuits are impracticable. The fact that metallic circuits have been laid over the Andes, may be taken as proof that there are remarkably few land-areas that cannot be spanned by wires. For communication between *fixed* points on rough coasts, a wire suitably protected is still the right and the best thing, as is evidenced by the cable,⁴ laid in 1890 between Pollagill Bay on the north-west coast of Ireland, and Portdown Bay, Tory Island, and thence by duplicate underground cables to the lighthouse on the north side of the island. The great advantage of a metallic-circuit system is the consequent privacy of the messages, the simplicity of the apparatus, the speed of transmission, and the possibility the system offers for working by telephone, and in other ways avoiding the expense of skilled operators. Space-telegraphy is at present limited to comparatively short distances, and its usefulness is confined to spanning estuaries, skirting sea-boards, and for such purposes as

¹ "Improvements in Magnetic Space-Telegraphy."

² "Ætheric Telegraphy."

³ "Telegraphy by Magnetic Induction."

⁴ See an important paper by Mr. H. Benest, "Coast-Telegraph Communication," read before the Balloon Society, March 18, 1892).